

*The Freeman material came just as I was  
marking this. Thanks*

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Dr. Joshua Lederberg  
The Rockefeller University  
New York, NY 10021

*see Hayes  
papers*

Dear Josh,

Thanks for your letter of September 9, 1988. Your comments and queries are always welcome. I have completed a first draft of my chapter on mutation and am now well into the middle of the chapter on mating. I hope to get some drafts to you before Christmas. Your 1987 Ann. Rev. Gen. is useful, but I have several questions:

- 1) Why was Tatum senior author on the 1947 J. Bact. paper? I believe you had done all the experimental work, and you had been senior author on the Nature and CSH papers.
- 2) The Genetics 1947 paper must have been submitted about the same time as the J. Bact. paper. You are sole author on this paper. Why?
- 3) The fungal analogy for the bacterial mating results must obviously have been very strong, especially since both Tatum and Ryan had worked on Neurospora. I noted in your work the term "relative sexuality". This obviously must have had some influence on your interpretation of the Hayes work?
- 4) You have mentioned in several places of your early interest in medicine and some regret that U.W. Genetics was in Agriculture. Did you every have an interest in practicing medicine, or was your interest just in medical research? I have the impression that your medical interest is what pushed you in the direction of bacteria.
- 5) You asked what did Luria and Delbrück really prove? I have always thought that the fluctuation test was rather obvious, but it clearly started the ball rolling. Alderberg, Magasanik, and others have written that this was the watershed paper. One of my regrets is that the early Luria/Delbrück correspondence (pre-1947) is missing from the CalTech archives. I found a 1970's letter from Delbrück to Luria asking Luria to send back all this correspondence (which Delbrück claimed to have sent Luria for some reason). I wrote Luria about it and he denies ever having received the correspondence. Because Delbrück was such a detailed letter writer, there should have been some insights here. Luria's autobiography is of little use, since he wrote it from memory and made mistakes.

6) There are many slightly negative things about your early work in Delbrück's files. Since Delbrück corresponded with every one, he was a central repository of ideas, gripes, etc. Watson, in particular, was very gleeful about Hayes' work, mainly (I believe) because it cast doubt on your interpretations (we won't talk about Watson's "three chromosome model"!).

7) You mention Hayes and J/W misinterpretations in their early work. I intend to look into this in detail. I have been corresponding with Hayes and I enclose a document (unpublished, I believe) which he has written for the Royal Society. It has quite a nice history of the very early Hayes work, but not so much about the interpretations in 1955-57. You recall your letter to Science in 1955, in response to a report of the Wollman/Jacob work.

8) You have mentioned Sonneborn as an influence and there are many references to cytoplasmic inheritance in your papers and other early papers on bacteria. I hope you will agree that the whole idea of cytoplasmic inheritance confused bacterial genetics. It seems to me that Sonneborn's work was accepted primarily because of the force of his personality. (I have read Dave Nanney's article on Sonneborn and, of course, knew Sonneborn well from my 11 years at Bloomington. You have probably seen Saap's book on the history of this work.) What do you think now about how cytoplasmic inheritance affected the interpretation of the early bacterial work? I know your paper in Physiological Reviews where the term plasmid was coined.

9) I wonder if you have given any thoughts to the "cult of personality" as it influences scientific development in general and genetics in particular? Would you agree that Delbrück is valued far in excess of his contributions because of the sheer force of his personality. How much damage is done to developments in science by strong personalities who let their prejudices rage? In the same vein, one might think of Morgan's anti-chemical influence, and the lack of recognition of Herman Muller for many years, presumably (if Carlson is to be believed) partly because of Morgan's influence.

10) I am interested in whether the real breakthroughs (new paradigms) might occur primarily by the lone scientist working away out of the big centers of research. Would you say that you were on the fringe of genetics in 1946, even though Columbia itself was a major center? How respected was Ryan by the classical (Dobzhansky, etc.) geneticists?

At one time you asked me whether you had any competitors for the Wisconsin job. I am not certain whether I responded. The only competitor who appeared in the correspondence was Max Zelle!

You mentioned in your letter some ideas about Pasteur's early work. I have heard Gerald Geison from Princeton discuss Pasteur's work on the attenuation of anthrax and I believe that one cannot make any sense out of this from Pasteur's published papers. According to Geison (who had access to Pasteur's original notebooks, now in the Bibliotheque Nationale), Pasteur actually made what he did look a lot more clear-cut than it actually was. I believe it would be stretching things

drastically to adduce any hypothesis related to plasmid loss. At any rate, you might be interested in the enclosed page from a paper Emile Roux wrote in 1925, at the time that d'Herelle's work was all the rage, about the hereditary nature of attenuation.

I hope the above questions will not burden you too much. Obviously, I don't need an immediate answer, but would appreciate any thoughts you may have.

Sincerely,

Thomas D. Brock